The Science of Team Science

Assessing the Value of Transdisciplinary Research

S. Leonard Syme, PhD

Several years ago, I chaired a committee organized by the IOM to consider the success of our public health efforts to prevent disease. The resulting 493-page report concluded that we were not doing a very good job.[1] The committee offered 18 recommendations intended to improve this situation. The first recommendation was that we needed to develop a better balance between clinical approaches to disease prevention (presently the dominant public health model for most risk factors) and work that recognizes the importance of generic social and behavioral determinants of disease, injury, and disability. The second recommendation was that we needed to develop interventions that took account of a wide range of health determinants that operated at the individual, interpersonal, institutional, community, and policy levels. The main message was that we needed somehow to transcend our disciplinary silos and consider a much broader set of determinants in a far more complex way than we have so far been able to do. Easier said than done. The papers in this supplement to the *American Journal of Preventive Medicine*[2–16] therefore are a timely, important, and badly needed contribution to our work in preventing disease and promoting health.

We all know the problem. Within the next 15 years, the number of people aged >65 in the U.S. will have doubled. Medical care resources in this country are already severely challenged. When the number of older people dramatically increases, the burden on medical care will be beyond anything we can now imagine. The importance of disease prevention in helping to deal with this crisis is obvious. To develop appropriate and effective prevention programs is going to require a new paradigm.

At present, our prevention efforts depend on research to identify disease risk factors so that we can share our acquired wisdom with people at risk. The idea is that these people will then rush home and change behavior to lower their risk. There are three problems with this approach. First, it has proven extraordinarily difficult to identify those risk factors. For the leading cause of death, coronary heart disease, the major identified risk factors (serum cholesterol, high blood pressure, smoking, physical inactivity, obesity) account for less than one half of the coronary heart disease that occurs. Our success in identifying risk factors for other diseases is even less impressive. Second, even when risk factors are identified, it has proven very difficult for people to change their behavior to lower their risk. And third, even when people do successfully reduce their risk, new people continually enter the at-risk population because we rarely identify those forces in the society that cause the problem in the first place.[17] Our silo-based work has not served us well.

The challenge of overcoming this silo approach is overwhelming. Those of us in different silos have been trained quite differently, we have read different kinds of books, we use different languages, we evaluate the quality of research data and evidence quite differently, and we have very different assessments of what it takes to do good research. Oftentimes, we don't even respect one another. Can you imagine these types of problems in an environment where a specific problem needs to be solved? Imagine a company that makes airplanes. In such a company, there must be people representing hundreds of discipline specialties. It is inconceivable that these people would argue about the supremacy of their discipline compared to the others. They have an airplane to build! The challenge of solving the design and construction of the airplane problem clearly would take precedence over turf battles.

It is within the context of this charged and sensitive environment that we welcome this supplement to AJPM[2–16]: There is a paper in this volume that explicitly examines the collaborative process and the way it affects the trust and respect of participants. There is a paper that suggests ways to assess the collaborative process. There is a paper that presents examples of other areas in which transdisciplinary research has in fact worked well. There is a paper that examines the role of leadership in facilitating the transdisciplinary process. There are papers that demonstrate the ways in which transdisciplinary research has been useful in shedding light on the etiology of diseases, on risk factors, and on the translation of findings for more effective intervention programs. And there is a paper discussing the way in which interdisciplinary thinking has become an important dimension of thinking at the NIH.

From the Departments of Epidemiology and Community Health, University of California Berkeley, Berkeley, California

Address correspondence and reprint requests to: S. Leonard Syme, PhD, 577 University Hall, Berkeley CA 94720. E-mail: slsyme@berkeley.edu.

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This latter contribution regarding funding raises a pivotal challenge for the future of transdisciplinary work. The NIH is the most important source in the country for funding both research and training in the health field. Overwhelmingly, however, successful research and training grants are awarded for programs (1) that target a specific disease (coronary heart disease, cancer) or a disease-specific risk factor (smoking, obesity); and/or (2) that focus on work at the laboratory, clinical, or population level. Transdisciplinary proposals that seek to look at health more generally or that attempt to integrate work at several levels often have a difficult time in the traditional study section setting that dominates the review process at the NIH; that this landscape is now being reconsidered is refreshing and of critical significance for the future of transdisciplinary work.

Several years ago, the Canadian government decided to develop a National Institutes of Health for Canada. Many of us warned them that if they patterned their NIH along the same lines as our NIH, it would set back for many decades the cause of preventive work. They did subsequently establish the Canadian Institute for Health Research with the usual institutes devoted to cancer, circulatory diseases, arthritis, and diabetes but they also established institutes on population health, aboriginal peoples, health services and policy research, and gender. I served for 5 years on the Advisory Board for the Institute of Population and Public Health, and I can testify to the dramatically different type of considerations that take place when one is free to transcend a narrow focus on specific diseases and disease-specific risk factors. Similarly, the Robert Wood Johnson Foundation has recently developed a post-doctoral training program called Health and Society that specifically emphasizes a transdisciplinary approach to health. The work being done by many of these scholars is truly amazing. So it can be done.

Thomas Kuhn wrote is his classic book, *The Structure of Scientific Revolutions*,[18] that paradigm shifts occur in science when the old ways of making sense of the world are no longer useful or appropriate. The need for a transdisciplinary approach to the study of health and disease is critically needed because the traditional silo approach to these issues clearly is not adequate to the challenges we face. As has been noted, we are not able to identify many disease risk factors; even when we do successfully identify risk factors, it is difficult for people to change their behavior to change their risk profile; and even if people do change their behavior, new people continually take their place because we have failed to identify many of the fundamental societal forces that cause the problem in first place. A new paradigm is needed. The papers in this issue bring together a series of refreshing, imaginative, and urgently needed new perspectives on this problem. This supplement to AJPM is a major contribution to our thinking.

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